

Interactive comment on “Wintertime peroxyacetyl nitrate (PAN) in the megacity Beijing: the role of photochemical and meteorological processes” by H. Zhang et al.

Anonymous Referee #1

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The authors present two-month wintertime measurements of PAN, O₃ and related species at an urban site in Beijing. They observed decoupling in the variation of ozone and PAN concentrations. The data was analyzed in relation to meteorological transport (using wind and back trajectories and WRF model simulation). Then the wintertime concentrations of PA radical were inferred from the concurrent measurements of PAN and NO₂, and the formation and thermal decomposition rates were estimated to examine the net contribution of local chemistry to PAN.

There have been very limited measurements of PAN in China. This study reports the first PAN data in cold seasons, which will definitely enrich the literatures. The interpre-

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tation of data can be improved by pointing out the significance of PAN measurements in winter and by shortening some general discussions. I suggest publish this manuscript at ACP after the following comments are addressed.

Major comments:

The authors state that previous PAN measurements were all conducted in summer not in winter. Summer time is an obvious season to examine PAN and related pollutants in and around urban areas because of the strongest photochemical activities (producing highest concentrations of PAN and hence the largest impact on human health). The author should elaborate in the introduction the significance of the information on PAN in winter in urban areas, in terms of studies of atmospheric chemistry and/or concern on air quality.

The sections on data analysis should focus on some important processes. I found a lot of discussions in the present paper are rather general but have not yielded much new insight on atmospheric processes. I suggest some of these discussions (especially section 3.5) be shortened.

Specific comments:

1. Page 31877, line 17-24: Bernie and Krognes (2000) is cited, but this work is about PAN at a remote site. It is better here to compare the seasonal pattern in the present urban site with other urban sites. In urban areas, PAN should be at highest levels in summer.

2. Section 3.2: The author gives a detailed summary of previous measurements in Beijing and other places in China. These results all in summer time, making them different to compare with the present winter data, except pointing out a well-known fact that PAN is higher in summer in urban areas. I suggest removing this section; the authors can briefly compare the results with Beijing summer values in the end of last section.

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3. Figure 3: I suggest showing diurnal profiles of other related species, such as NO, NO₂, Ox, T, etc. This will help understand the diurnal variations of PAN and O₃.

4. Page 31879, line 25-26: The statement has no supporting data/augment from the authors.

5. Section 3.4, first paragraph: This part needs to be re-written. When comparing the scatter plot of O₃ and PAN, it is important to keep in mind that the present study is conducted in cold periods and in an urban area. Thus titration of O₃ by NO is important. As a result, the lack of correlation between PAN and O₃ is not surprising. When examining O₃ in urban area where titration is significant, one should use O₃+NO₂ (Ox) which takes into account of O₃ titrated by NO. In fact, PAN and Ox shows a good correlation with PAN (Fig 5), as expected. So the discussion of the apparent differences of PAN-O₃ in Beijing and other places do not provide any new information. I suggest re-writing and condensing this and other related parts.

6. Page 31881, line 13-14: "NO₂ is a better indicator of PAN plume. . .". I disagree this statement, as Ox should be used for O₃ in this case, and it is a good indicator as well.

7. Page 31881, last paragraph: The anti-correlation of O₃ and NO₂/NO in urban areas is well known, I suggest omit or shorten this part.

8. Section 3.5, discussions of wind speed and direction: I found this section contains a lot of general and redundant discussions. I suggest the authors significantly shorten this section to clearly indicate the key points they wish to make. When discussing the impacts of wind on the measured pollutants, there is no need to separate the wind speed and direction as they are inherently related with each other. As shown in Figure 8, the westerly winds generally show higher speeds. In the later part of the section on PAN and O₃ lifetimes, it is not surprising to see much shorter lifetime for O₃ due to strong titration of NO, again one should focus on Ox rather than O₃.

9. I don't see the need for section 3.6, as the proceeding section has already discussed

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the impact of air-mass transport (winds and back trajectories) on PAN concentrations. This section can be condensed and combined with section 3.5.

10. Section 3.6: It seems that in this section the authors wanted to estimate the rates of formation and decomposition of PAN and then the net chemical formation of PAN. If this is the case, make this purpose clearer in the beginning of the section. I further suggest including the observed rate of change of PAN in Figure 15, this way one can see the relative contributions of local chemistry and dynamic transport. This section can be made more concise.

11. In Fig 2, the unit of CO (ppb) is incorrect, should it be ppm?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 31871, 2012.

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